the world, the results of which will be published in due time.

The first and most striking result is that the solar radiation exerts a cumulative effect on the atmosphere so that prolonged periods of high or low solar radiation have a much greater response in atmospheric action than shorter periods of greater intensity. Thus a deviation of 5 per cent. from the average radiation of short duration might not produce so great an effect as a long period, with a mean deviation of 2 per cent.

The second result is that as the sun changes from one hemisphere to another the effect on the weather changes, so that in the hemisphere where the sun is nearly vertical the pressure falls over the land surface and rises over the water surface, while the opposite effect is found in the other hemisphere. This effect I take to be a proof that a considerable part of the increased solar radiation reaches the earth's surface and intensifies the normal effect of absorption of solar radiation by the land. There are, however, clear indications that a considerable part of the increased solar radiation is absorbed by the upper air and gives origin to atmospheric waves which, if they are not the determining cause, at least are very influential in determining the discontinuities referred to recently in NATURE by Prof. Bjerknes.

These atmospheric waves are of a complex nature, and for this reason the correlations with solar changes are best seen by separating the solar and weather changes into shorter and longer (or slower) waves.

By taking averages of ten days we found for 1916 correlations as high as 84 per cent. with certain Argentine stations, and correlations exceeding 80 per cent. in later years. The shorter waves do not give such high correlations, apparently for several reasons, the most important, perhaps, being that there are still some errors in the solar values due to the difficulties of eliminating the effect of changing transparency of the air while the solar observations are being made.

This difficulty has undoubtedly been greatly diminished during recent months by the use of the pyranometer, which enables the observers to get measurements of the solar heat from a single bolograph. Certainly since this method was begun the correlations of the solar variations with atmospheric waves have increased. Another difficulty appears to be that while the waves start from definite centres of action, these centres shift position to some extent, so that the time interval of the effects following solar changes is variable, and this variability is of greater importance in the shorter waves.

The prolonged effects following unusually high or low values of radiation shown by Dr. Abbot's curves, reproduced in NATURE, I believe to result from the normal sequence of events on the surface of the sun. After a marked excess or defect of radiation there is likely to follow a similar deviation from the normal after an interval of ten to thirteen days.

This result I believe to arise from the fact that when there is a marked outbreak of heated solar gases on the edge of the sun, where absorption is normally great, the total radiation is more intensified than when the outbreak is near the centre. If the outbreak is near the edge it will be carried by rotation to the opposite edge in from about eleven to nineteen days, according as to whether the position is on the east or west edge.

In the same way cooled gases produce their greatest absorbing effects when near the edge of the sun, and there is the same tendency to repeat. These effects also tend to repeat themselves after a solar rotation, but in that case the interval is so long that marked changes have usually occurred. However, these repetitions are sufficiently numerous to make it practically certain from the length of the rotation that the conditions causing the marked deviation from normal are in the region of the prominences, and not in that of the sun-spots, which have a period of about twenty-seven days.

These studies of the relations between solar changes and the weather have been in progress in the Argentine Meteorological Office for several years, so that when a station of the Solar Physics Observatory of the Smithsonian Institution was opened in Chile, arrangements were made between Prof. G. O. Wiggin, director of the Argentine Weather Service, and Dr. Walcott, director of the Smithsonian Institution, for the transmission of the solar radiation measurements by cable to Buenos Aires and for their use so far as possible in weather forecasting. To this work Prof. Wiggin and the assistant chief have given enthusiastic support, and through their efforts, and as a result of a growing interest in the subject in Argentina, the Legislature has recently appropriated some 25,000 pesos for the installation of a solar observatory in Argentina.

The forecasts are made for a week in advance by publishing estimated temperatures for each day, and also forecasts of expected rains. With increased experience there have been steady improvements and an increasing demand for the forecasts by commercial interests.

There are many complexities which remain to be solved, and perhaps some which can never be solved, but I believe that a distinct advance has been made in forecasting, and the progress is too far advanced to be turned backward. One of the greatest difficulties is the inversion of the solar relation with the season, and occasional inversions for other reasons not yet clearly understood.

Heretofore, most of the studies by others in regard to the relation of solar changes and atmospheric phenomena have been in reference to the sun-spots. Dr. Abbot's observations indicate clearly that there is a change of solar radiation corresponding to the eleven-year sun-spot period. The radiation increases with the increase in spottedness, but this change is of relatively small importance compared with shorter changes of greater intensity.

Sir Norman Lockyer suggested some years ago that there were weather changes due to solar changes other than the eleven-year period, and brought evidence to show that a period of about four years in the outbreak of prominences caused atmospheric changes having the same period. But solar radiation shows a variability far greater than is indicated by the observations of prominences, and the high value of the correlation between these and the weather which we have found for the Argentine and for other regions of the world leads me to believe that these changes are the chief, if not the only, cause of weather changes as distinct from the well-known diurnal and annual periods.

H. H. CLAYTON,
Chief of Forecast Division,
Oficina Meteorológica Argentina.

The Physical Meaning of Spherical Aberration.

THE nature of the distribution of light round the axial focus of a lens is a problem which has frequently been attacked mathematically. It has been quite successfully solved for points in the principal focal plane of an "aplanatic" lens when the intensity is found to depend on functions of the form (assuming Huygens's principle)

$$\text{Intensity} = \text{const} \times J_1^2\left(\frac{2\pi k}{\lambda} \quad \frac{a}{R}\right),$$

where J_1 is the Bessel function of the first order, R the radius of the wave-surface at the lens, a the semi-aperture, and k the distance of the point from the axis. At present it has not been found possible to give an expression for the light distribution in the presence of spherical aberration, cr away from the focus, which can be physically interpreted in such a simple way. Prof. Conrady has been able, however, to determine the distribution numerically (Monthly Notices, R.A.S., vol. lxxix., No. 8) in a series of simple cases by mechanical quadratures which give the value of the integral

$$I = \text{const} \times \int_{a=0}^{a=a_1} \{J_0^2(\alpha a)\cos^2\eta_a + J_0^2(\alpha a)\sin^2\eta_a\} d(a^2),$$

where

$$\alpha = \frac{2\pi k}{\lambda R},$$

η representing the phase which is supposed to vary in a spherical reference-surface by an amount depending on the spherical aberration. Prof. Conrady assumes a series of likely cases for phase distribution. No analytical expression for η can be obtained for any but the simplest optical systems, but empirical expressions can easily be derived from the result of trigonometrical ray tracing or Hartmann tests in the most complex systems, thus enabling the truth of the numerical results for light distribution to be checked experimentally. The value of

$$\eta_a = \frac{2\pi}{\lambda}\int_0^a \theta da \text{ (very nearly)},$$

where θ is the angular aberration derived from the calculated or observed lateral aberration or lateral intercept due to displacement from the centre of the spherical reference surface.

It is not too much to say that the thorough solution of the problem is of the greatest importance in the study of the performance of optical instruments. In order to search for the actual phenomena predicted by Prof. Conrady in theoretical cases, and further to explore the subject, I have recently carried out a critical examination of the image of a very small source of light (a fine "pinhole" in a silver film) produced by a microscope objective having excellent spherical correction, and for which the curves, glasses, etc., were known. The spherical aberration introduced by varying the tube-length can thus be calculated for any conditions. A nearly linear relation was found between the phase difference of the paraxial and marginal rays at the marginal focus and the reciprocal of the tube-length.

As a check on the calculation, I was able to devise a method of performing a test on the microscope objective very similar to the well-known Hartmann test employed for telescope object-glasses. By this means the properties of the objective became well known.

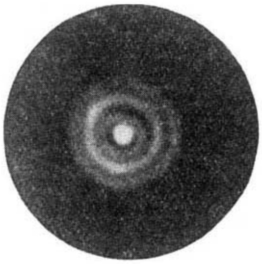
The changes in the distribution of light at the best focus in the presence of varying amounts of spherical aberration have been examined quantitatively, both visually and photographically. A perceptible loss of light from the central disc, estimated at 20 per cent., occurs when the residual aberration at the best visual focus amounts to 0·25λ. Such light is scattered into the surrounding field; it does not appear in the first bright ring at this focus. The sizes of the rings in the diffraction pattern at the best visual focus do not depart measurably from the theoretical values in the presence of residual aberration amounting to 0·6λ at the focus where there is least confusion of phase.

The "out of focus" appearances present many points of great interest. It appears that the successive bright rings retain a marked individuality, but suffer periodic variations in brightness and "thickness." This causes the dark rings between them to suffer corresponding variations in "darkness" and in radius. Under a high magnification the familiar broad, dark diffraction rings which appear to grow in the expanding "out of focus" disc are found to grow as the result of this periodic motion of the smaller dark interference rings, the whole action resembling that of a model to illustrate the propagation of the compression waves of sound.

If, when the aberration is a minimum, we go sufficiently far out of focus to introduce a path-difference between marginal and paraxial rays, $dp = 0{\cdot}5\lambda$, on either side of the focus, we find that the first dark ring has nearly filled with light. At about $dp = 1\lambda$ the central disc has lost all its light, and the first bright ring is at a maximum. At about $dp = 1{\cdot}5\lambda$ the central disc has again reached a maximum, the first ring is at its minimum, the second at a maximum, and the third nearly equal to the second. We thus see the first broad, dark diffraction ring between the central disc and the annulus of light formed by the second and third bright rings. So the various changes progress, the location of the successive bright rings being given fairly nearly by the ordinary theory. When, however, a definite amount of *aberration* is introduced, sufficient to cause a residual variation of phase of 0·5λ at the best focus, the changes are violently dissimilar on the two sides of the focus—a fact which is fairly well known. On one side there is a quick dissolution of the central concentration into a mere haze, while on the other a bright and well-formed ring system is found in which the broad diffraction rings spread out with much the same action as before, except that the periods of the variations are altered from those in the "no aberration" adjustment. Further, on this side of the focus, as was suggested by Prof. Conrady's numerical results, a central concentration persists which diminishes considerably in size as compared with the "best focus" disc, but remains brighter than the rest of the ring system up to a path-difference $dp = 3\lambda$—a displacement in the actual focussing point of 7 mm. in a total tube-length of 23 cm. It can easily be seen that this effect is quite capable of rendering possible instrumental performances, so far as resolving power alone is concerned, far in excess of any value possible in the absence of spherical aberration, although this would be a perfectly legitimate conclusion only for such cases as that of a double-star resolution by a telescope objective. A fairly complete set of photographs to illustrate the various appearances has been made. These have been measured and examined for the purpose of intensity determinations.

The importance of these matters lies in the determination of the effects of aberration, expressed in ray intercepts, on the distribution of light in the image, as the distribution suggested by ray concentration is often nothing approaching the truth. The "out of focus" appearances, too, are of great importance in dealing with "roundness of field." It is possible, in the light of such results, to form ideas as to legitimate tolerances in design and manufacture.

The investigation, to become complete, must be extended to other types of aberration, but it is hoped shortly to publish a complete account of the experimental work, of which the foregoing *résumé* may give an idea of the scope. The photograph herewith shows the ring system when a residual aberration, in the sense of "over-correction," of $0{\cdot}5\lambda$ is present at the best focus, and when the reference plane is taken sufficiently far outside the focus to make the path-difference $dp = 1{\cdot}8\lambda$. The small central concentration and the persistent individuality of the interference rings are shown. The first "broad" diffraction ring is now formed through the low intensity of the first bright interference ring. The whole of the above work relates to nearly monochromatic light only.



A good many of the broad features of these aberration and "out of focus" effects can be explained without difficulty on the basis of the Fresnel zone theory, and it is hoped to include a short discussion of this kind in the paper. No more need be said at present except to direct the attention of the readers of Nature to the importance of the paper on "Star Discs" to which the reference is given.

L. C. Martin.

Imperial College of Science and Technology,
November 25.

"Phenomena of Materialisation."

In Nature of November 18 I find what purports to be a review of Dr. von Schrenck-Notzing's work translated by me under the above title. I have always supposed that a reviewer should tell the reader what the book is about. The review in question is headed "The Newer Spiritualism," and begins: "'Of making many books' on spiritualism 'there is no end.'" It states that the phenomena are alleged to have "a spiritistic interpretation," and refers to "the numerous photographs of her [the medium] sandwiched between faked spirit photographs." All this is thoroughly misleading. The book is not about spiritualism. Both the author (p. 30) and the translator (p. x) discard the "spirit hypothesis" as unnecessary. The author says that "it impedes and hinders in every way serious scientific investigation." The book contains no portrait of the medium, and not a single "spirit photograph," faked or otherwise. The photographs reproduced have not been manipulated in any way except Nos. 127, 128, 134, 136, 138, and 140, in which, for purposes of publication, the sex characteristics have been obliterated. These six photographs are marked "retouched," and the reason for retouching is stated in the text.

The review abounds with other inaccuracies. A red light was not always employed (see p. 306). Mme. Bisson did not "hop in and out" of the cabinet. The rare occasions on which she entered it are stated, as are all the other conditions, with what the reviewer calls "dreary uniformity," but with what the ordinary scientific reader would call conscientious accuracy.

The reviewer's challenge to "exhibit" the phenomena in London before well-known hostile critics is about as reasonable as to ask a performing mouse to exhibit its tricks before an audience of hungry cats. The psychological element would probably produce similar inhibitions in both cases. The phenomena are nearly as rare as the fall of a meteorite from the sky, and nearly as spontaneous as the production of biological "sports." They cannot, therefore, be produced before a massed audience. It is useless to think that any living individual is eminent enough to carry a general conviction of the reality of the phenomena, even though he be personally convinced. The only resource is to take the phenomena as and when they come, to record them carefully, if possible by photography and other instrumental means, and to make the experimental conditions gradually more rigid until we can only conclude that we are face to face with a new set of biological phenomena exhibiting the known powers of the human organism in an intensified and much accelerated form. And this is precisely what Dr. von Schrenck-Notzing has done.

E. E. Fournier d'Albe.

It will be well to deal with Dr. Fournier d'Albe's charges against my accuracy *seriatim*:

(1) "Both the author (p. 30) and the translator (p. x) discard the 'spirit hypothesis' as unnecessary." I might have made this clearer, but the repudiation is not easy to reconcile with the contents of a book crammed with references to occult phenomena, as, *e.g.*, mediumistic, psychic, and telekinetic.

(2) "The book contains no portraits of the medium, and not a single 'spirit photograph.'" This is an amazing statement. There are 225 illustrations. Of these there are 13 "drawings," so-called; all the rest are flashlight photographs of the medium (if not, then of whom?) in various attitudes, a large number showing the teleplasma issuing from her mouth, etc. There are 20 flashlight photographs—"phantasms" they are called—of dead and living people. Among the former Mme. Bisson recognised the features of a deceased nephew, Georges Thurner, and also of her husband, who died in 1912.

(3) "A red light was not always employed (see p. 306)." "All the sittings took place in a red light, so that during the four years there was not a single dark séance" (p. 21). The translator may be left to reconcile this statement with the exception to which he gives the reference.

(4) "Mme. Bisson did not 'hop in and out' of the cabinet." Probably she neither hopped nor skipped, but her visits to the cabinet were frequent enough to arouse suspicion as to collusion with a medium over whom she had "absolute control" (p. 59). Dr. Fournier d'Albe does not appear to have been present at the sittings.

(5) "The reviewer's challenge" cannot be accepted because the phenomena cannot "be produced before a mixed audience. . . ." "We are face to face with a new set of biological phenomena." So long as those who assert their belief in teleplastic exudations from the body and in the genuineness of photographs of the dead refuse to submit these "new biological" phenomena to the conjoint judgment of men of science and conjurers, they must not be surprised that their so-called "evidence" carries no weight save among the credulous.

The Reviewer.

Higher Forestry Education for the Empire.

A correspondent has sent us some remarks upon Prof. Stebbing's letter dealing with forestry education in Nature of December 2, but he has omitted to give his full name and address. No use can, therefore, be made of his communication.—Editor, Nature.